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THE JOURNAL OF PHILOSOPHY

THE INDEPENDENT VARIABILITY OF PURPOSE AND BELIEF

THAT there is no purpose without cognition may be taken for granted. Shall we then slur the difference by using the terms interchangeably, or by introducing blends such as "appreciation," "judgments of practise," and "value-judgments"? On the contrary, where there is intimate complexity the sound method is that which analyzes and distinguishes. One can not expect to follow two closely interwoven strands until one has first clearly identified each of them; and in that preliminary stage of identification it is necessary to dwell upon differences, rather than to slur them. This is quite consistent with a full recognition of their intimacy. It presupposes that intimacy. There is a problem only because of such intimacy. Interest affects belief in countless ways, by directing it to a certain context, by accompanying and impregnating it with attitudes of favor or disfavor, and by weighting the evidence on which it is based. Belief, on the other hand, affects interest, by exciting or depressing it, by knowing it, by illuminating it, or by determining the forms in which it expresses itself. But how can we say these things without implying that belief is one thing and interest another? And how can we understand these complexities, relations and interactions without some preliminary understanding of the terms themselves?

The separability and independent variability of these two factors is assumed by common sense, as is indicated by such expressions as the following: "I am compelled to believe" (by implication "against my will"); "I have reluctantly concluded;" "I was agreeably disappointed;" "I am sorry to find;" "I wish it were so;" *etc.* The child who sees his mother enter at one door with a bottle, and his father at the other with a slipper, doubtless does so with feeling, but none the less he entertains *beliefs* or *expectations* with reference to both stimuli, as truly as does the unfeeling psychologist who stands by to observe the phenomenon. One may take a belief and show how it may remain unaffected through the whole repertory and cycle of the passions, including the point of indifference. When an historical event such as signing of the armistice is made known, there oc-

curs a moment of belief which may in individual minds be combined with joy or grief, with rage or fear, with pleasure or displeasure, and with any degree of emotional or affective intensity. A given individual having adopted a belief with reluctance, may later be glad, and eventually not care. In passing through these phases the individual has never changed his belief or wavered in his adherence to it. Similarly an interest may remain fixed while belief varies. One may desire peace and not falter in one's pursuit of it, while belief in its achievement passes from disbelief through doubt to ascending degrees of certainty. Let us now seek to interpret this independent variability more exactly.

In order to understand the difference between interest and belief it will be necessary to consider a common constituent which with one qualification becomes interested action, and with another qualification becomes belief. This more fundamental process may best be called "supposition."[1] It is essentially an anticipatory set, or implicit course of action correlated with a specific object. If I suppose or entertain the idea that the barn is on fire, I in some measure set my fire-response in readiness. I talk to myself in terms taken from my fire-vocabulary; and I am peculiarly receptive to such visual or other sensory stimuli as fire has in the past presented. Other trains of anticipatory responses such as fire itself is most apt to excite are now partially excited in its absence. In short, a reaction-system of which fire is the complementary environmental factor is in momentary possession of my mind. This may be the end of the matter, so far as this particular system is concerned. In the course of the implicit elaboration of this response some other system may have been started into action, and I may wander from supposition to supposition through a more or less protracted sequence of "idle" conjecture.

If we examine the structure of a supposition we find a further important property. It is *scheduled* or set *for* a specific occasion or class of occasions. Supposing my barn to *be* on fire is, for example, a readiness for fire-stimuli when I now at this moment look out of the east window rather than when to-morrow I look out of the west window. The supposition determines not only what my response shall be, but on what occasion it shall be applied. Having a supposition that my barn is on fire, there is some specific situation which will find me by virtue of that supposition on the look-out for fire and

[1] This is approximately the same as the *Annahme* of A. Meinong, who deserves credit for having brought this process to light. The present treatment, however, differs radically from Meinong's; especially in getting away almost wholly from the traditional form of the judgment or proposition, and in giving the matter a schematic *physiological* interpretation.

ready to deal with it. By virtue of my supposition I shall be more or less ready to act according as fire is or is not presented on *that* occasion. If fire is presented, I shall be able to respond to it without preliminary adjustment, and shall be in advance of another to whom, as we say, no such possibility has "occurred." *Per contra,* if fire is not presented my response is, so to speak, thrown back upon its haunches. I am taken by surprise and have perhaps a more difficult readjustment to make than another who has not thought at all.

It will be convenient to employ the term "index" for the stimulus or situation which brings the response into action. In the formal judgment the so-called "subject" is the index, the so-called "predicate" is the response, and the fact whose presence or absence determines the judgment to be true or false is the complementary environmental condition or "object." The subject is given, the predicate is applied and the object is contingent. The index may assume a variety of forms and in a given case it can be identified only functionally. In the supposition considered above the subject is not "my house" in any determined verbal, imaginal or physical sense. The subject is that situation in which my readiness for fire matures and is brought to bear. In any given case the same supposition might equally well be expressed as "If you will look through that window you will see flames." When a judgment is formulated verbally the so-called "subject" ordinarily *instructs* the organism to which it is addressed. It localizes or sets the attention, and determines what stimulus shall serve as index. In some cases a word may serve both to instruct and to indicate. If the word "fire" is shouted in my hearing with a certain intonation my fire-response is aroused at that moment and at that place. But the words, "There is a fire out there" prepare me for what I further see (the complementary stimulus of red flames, *etc.*) when I received visual stimuli with a certain specific localization (the index). "Your house is on fire" may determine this same index, but it will also determine me to look elsewhere for evidence, as *e.g.,* in my automatic fire-alarm, or the report of my stable-boy. In other words the latter judgment has a wider range of verification, it establishes more occasions of adjustment or of maladjustment, of surprise or of confirmation.

Truth and error are said to be relative to the intent of a judgment. I know of no better description of this than in the terms here proposed. Truth and error qualify an anticipatory set as regards *its fitness to meet a specific occasion.* There can be no determination of its truth or error so long as the locus of its application remains ambiguous. A charge of error can always be effectively

met if one can show that "*that* was not what one was talking about"; that is, if there remains doubt as to where the evidence is to be sought. By fitness is meant only that the anticipatory set does or does not there find the complementary object by which it can move to completion. This "complementary object" may be a single actual stimulus, or a group of stimuli, or a group of physical properties, by which a response is enabled to execute itself. Fire as an object consists of the way it looks, sounds or feels; and also of the other things that can be "done" with it, such as burning things with it, or quenching it. In fact since my response to it may consist in part of a further extension of plans, the object must include its capacity to mean or to act as a sign. Object, in short, is much more than stimulus.[2]

A bare supposition does, then, have the functional relations that are necessary in order to determine truth or error. There is need of much greater elaboration before this problem can be said to be adequately solved, but to pursue it further here would take us far off our charted course.[3]

[2] *Cf.* E. B. Holt's doctrine of "the recession of the stimulus," *The Freudian Wish*, 1915, 75 ff.

[3] For a further discussion of aspects of this problem, *cf.* my article entitled "The Truth-Problem," this JOURNAL, 1916, Vol. XIII, pp. 505, 561. Mr. Russell has recently undertaken to describe thought in terms of "images," a reversion to a mode of epistemology that has been showing symptoms of obsolescence. Although I sympathize heartily with Mr. Russell's appeal to "fact" as the test of truth and error, I can not see that he has escaped any of the difficulties and shortcomings of the "representative" theory. It is not a question of whether "images" in some sense exist or not; doubtless they do. But, in the first place, their nature and conditions are so obscure that it is good strategy to attack somewhere else in the hope of understanding them better by taking them in the rear or in the flank. And in the second place, they do not appear to play the part in thought which Mr. Russell attributes to them; inasmuch as: *first*, their presence is not necessary, even though usual; *second*, their resemblance to the "object" is not necessary, though common; *third*, even where there is an image which is similar, this does not sufficiently describe the thought's selection of its object, its pointing or reference, since error, in other words dissimilarity, does not make a thought any less a thought. *Cf.* B. Russell: "On Propositions: What They Are and How They Mean," in *Problems of Science and Philosophy, Aristotelian Society*, Supplementary Volume II, 1–43. Mr. Russell's emphasis on images has exposed him to H. H. Joachim's attack in *Mind*, 1920, XXIX, pp. 404–414. In the same number of *Mind* (398–404) appears a restatement of Mr. Russell's view with a great difference at least of emphasis. He here makes little of images; says that "the essence of meaning lies in the causal efficacy of that which has meaning" (398); and that meaning attaches to "signs," which are "sensible (or imaginal) phenomena which cause actions appropriate, not to themselves, but to something else with which they are associated" (402). With this revised view I should differ only in points of detail. It illustrates, I think, a fundamentally sound method of attacking the question.

Commonly a supposition is further qualified as: (1) a belief; or (2) a purpose.[4]

1. A belief is a supposition to which one has committed oneself. This is evidently a matter of degree. In every supposition there is some degree of belief. Doubt is feebleness or vacillation of belief; and disbelief is contrary or antagonistic belief. There is some belief in all supposition because all supposition is action, and action which precludes other action. When one is following up, exploring, or elaborating a certain supposition, other suppositions are cut off, and for the time being one is committed. But for practical purposes it is easy to distinguish such momentary and innocuous committal from irretrievable committal, in which one's bridges are burnt behind one. Irretrievable committal is the case in which the supposition has gone so far as to exclude all other suppositions with the same index. There are no mental reservations, no anchors to windward. The non-occurrence of the complementary objects finds one utterly maladjusted. This condition may develop in various ways. If upon supposing my barn to be on fire I cry "Fire!" telephone for the police and rush out of the house in the direction of the barn, I am engaging in activities which for the time inhibit the supposition that the barn is in a state of "normalcy." I am also creating a rapidly shifting series of new situations to which I must react as they arise, and which prevent vacillation, that is, the alternation of the two major suppositions. Or I may have carried my supposition so far as to make it impossible to reverse because of the momentum of the response. Even though the complementary stimulus should not appear the organism would be caught off its balance and unable to bring another response into play. Or a supposition may have been carried so far as to lead to amendments in other systems of response. Supposing my barn to be on fire I give up my trip to the city and suspend household activities, so that the contrary supposition is impeded through requiring a general rearrangement of plans. Or the supposition may come in the shape of a suggestion, which, finding an unresourceful

The great merit of pragmatism lies in its having discredited the image theory of knowledge, and in its having introduced a functional view of meaning. James, *e.g.*, speaks of meaning in terms of "experiences to be expected" (article on "Pragmatism" in *Dictionary of Philosophy and Psychology*). But this function must remain more or less mythical until it can be interpreted in terms of organic behavior; and unless it presupposes a physical environment.

[4] Or it may be a hypothesis, a question, a doubt, a command, a wish, *etc.* While we must for the sake of brevity omit these variations from the present discussion it would appear that up to a certain point they are all alike, and homogeneous with the acts of mind considered above.

and unresisting mind, may obtain exclusive possession by default.[5]

2. Purpose has been elsewhere examined in its own terms,[6] but its nature now requires a brief restatement here in the light of the nature of supposition and belief. In purpose a reaction-system being partially aroused generates auxiliary activities. These may be random activities of a type likely sooner or later to provide the complementary object by which the response is completed; or they may be activities which owing to previous experience *promise* that object. Where the former is the case we may speak of longing, wishing or craving, as present; but only where the latter is the case may we properly speak of purpose.

A purpose, then, requires the presence of a supposition, which ordinarily will have assumed the form of a belief.[7] The auxiliary act is performed because the belief which qualifies it is an anticipation of the response required for the completion of the determining tendency. It follows that a belief becomes a purpose only when the anticipatory response in which it consists is *in demand*. The belief correlates the anticipatory response with a specific occasion; the purpose subordinates it to a determining tendency. In the case of purpose the determining tendency and the component belief are so related that one can be inferred from the other. If the agent is asked to justify his action in terms of his interest, he will state his belief; if he is asked to justify his action in terms of his belief, he will state his interest. Thus if an ambitious man who allows himself to be interviewed by a reporter is asked to explain, he will explain that he is ambitious if he supposes you to be familiar with his beliefs regarding the political effects of publicity, and he will explain that it pays to advertise, if he supposes you to be familiar with his ambition.

Belief and purpose may also be linked through containing the same component response. What I believe about a situation may consist of what I propose to do about it. Thus in the case of the aforesaid fire, the believing consists in part of the arousal of my fire-extinguishing activities, and these activities fully aroused become my method of dealing with the situation and express my desire with reference to it. The belief is the fact that my mode of

[5] There are undoubtedly other factors in belief, such as the "sense of reality," which I think can be interpreted as the receptive attitude to external stimuli; and the factor of earnestness or zeal, which is a blend of belief with resolution or will.

[6] *Cf.* my article "A Behavioristic View of Purpose," this JOURNAL, Vol. XVIII, No. 4.

[7] One may act on doubtful grounds; which would mean that although a plan is being carried out alternatives are not wholly abandoned.

dealing with fire,[8] whatever that may be, *is now brought into play*, to the exclusion of my modes of dealing with the safe and usual condition of my property. The purpose to put the fire out lies in the *character* of my fire-response, and its *selective control of my action.* I do what I do because of its paving the way to my fire-response, in other words, I have a purpose with reference to the fire; and that response consists in movements culminating and coming to rest in the experience of fire-extinguished, in other words, the purpose in question is to put the fire out. That I act on fire as I do, and that I act at all is a matter of purpose; but that this mode of acting on fire should be correlated with a specific situation, that I should bring that rather than some other response to bear here and now, is a matter of belief. The force and quality of my act are derived from purpose, its *opportuneness* from belief.

We may now understand more clearly the alleged impotence of reason. It is the practical function of reason or the intellectual faculties to effect certain internal adjustments by which preformed unit-responses are fitted to a governing tendency; that is, to find among the individual's existing propensities the means by which a purpose may be executed. Belief without purpose would refer to explicit action only hypothetically. It would mean an established connection between an indicated situation and a mode of response, which would render the indicated situation eligible in case there was any call for the response. What belief does is thus to establish connecting channels by which the currents of purposive energy are distributed and directed. In the absence of such currents, belief is like an empty aqueduct.[9] But this is after all no more than to say that belief is one of the conditions of action. When it *does occur*, it *is* a condition; and owing to its peculiar controllability, it may for practical purposes be the all-important condition; as important, for example, as the agencies by which "physical" forces are directed and applied.

The distinction between purpose and belief is further reflected in the distinction between motor-affective meanings and cognitive meanings. A datum derives meaning from the present and impending

[8] In a first experience of this sort modes of response will be integrated in a new way. I am prepared for what I have never met before, for example, fire on my own property. I have modes of dealing with fire, and with my own property. These being combined in advance create a readiness for a novel situation. All beliefs doubtless have *some* element of novelty in them.

[9] I do not mean here to take a stand either for or against the "ideo-motor theory," according to which a belief if left to itself will take effect in action. Cases of pure ideo-motor action are in any case rare; and even if they do exist the all-important fact is that the belief *is* left to itself, that it does, in other words, function as a determining tendency.

action which it arouses. A pain-stimulus is immediately qualified by the response of rejection and may be said to acquire the meaning of intolerability, as food being or about to be eaten acquires the meaning of edibility. The meaning consists in the imminent action already formed and started on its course. But a datum has cognitive meaning only in so far as it arouses a conditional response, that is, a response whose completion is contingent on the further development of the situation. It is not necessary to suppose that the one of these meanings is possible without the other, but only that they are distinguishable. The motor-affective meaning is infallible, the cognitive meaning is liable to error. If I have already begun the activity of eating, then the object so responded to possesses in some measure the quality or value of edibility. *I am finding it edible.* If I anticipate the later stages of the process but find it impossible to carry them out, if the object can be carried to the mouth but can not be swallowed, then it belies its promise and I have judged in error. Motor-affective meaning is the existing response or its completion in so far as these require no further development of the stimulus; cognitive meaning is projected response correlated with a future series of objects which may or may not be presented. The confusion of these two types of meaning has led to the error of regarding motor-affective responses as a kind of true knowledge, or to the error of regarding some cognitive responses as having the infallibility of fact.[10]

In order further to illustrate the interplay of interest and cognition let us now examine some of the modes of their independent variability. There is, first, the case in which belief remains fixed while purpose varies. I expect a visit from X to-morrow at four o'clock. My attitude to X being one of enmity I schedule my combative response for that hour. Meanwhile I experience a change of heart about X and come to regard him as a friend in disguise. What I then change is the character of the X-response. My expectation of his arrival is unchanged; that is, my X-response, which is now a grateful response, is still scheduled for to-morrow at four o'clock. My surprise at his non-appearance will be in no way affected by the

[10] I take it that the view here proposed is approximately the same as that of Mrs. De Laguna, formulated in her valuable article entitled "Emotion and Perception from the Behaviorist Standpoint," this JOURNAL, Vol. XVI (1919), pp. 409–427. She says, for example, "In so far as a stimulus is calling into play a specific type of response, belonging to a single genetic and functional system, it possesses the affective quality experienced in emotion; in so far as the stimulus calls into play an attentive postponement of response, it arouses cognitive awareness and possesses perceptual quality" (421–422). Mrs. De Laguna also has in mind the analytical function of perception.

change in the greeting which I have prepared for him. Or consider identity of belief with variety of purpose in different individuals. The rumored signing of the armistice on November 8, 1918, was greeted by some with delirious joy, by others with rage. The defeat of Germany was already linked with different reaction-systems in the minds of these two groups. The one group struggled to bring it about, the other to avert it. The identity of belief lay in the fact that both took the same occasion to discharge these different reaction systems; and both were equally in error, in that in both cases the response was equally premature.

Second, there is the case in which there is stability of purpose with variability of belief. A mother loves her son with steadfastness. This means that she is disposed to rejoice at his success and to grieve at his failure. She will then rejoice or grieve in accordance with her beliefs. She hears a report of his success and believes it. This means that she responds to the situation with the response that constitutes her way of greeting her son's success. The father, being more skeptical, suspends his rejoicing, though he is no less disposed to rejoice at his son's success when once he believes it. Additional evidence then leads to the mother's abandonment of her belief, that is to the abandonment of her rejoicing. Her rejoicing turns out to be ill-timed. The situation is one to which her grief rather than her rejoicing is appropriate. Meanwhile she has not in the least changed her sentiment, that is her system of reactions in relation to the fortunes of her son. She has revised only the schedule, the timing, the application of these reactions. Similarly the sentiment of humanity may beget in one man a chronic melancholy, in another a spirit of joyful service. The difference between the pessimist and the optimist is not a difference in what they love and hate, but a difference in what they think, that is a difference in the occasions on which they bring their love and hate to bear.

Third, there is the case of a converse relation between belief and purpose. In hopeless longing there is a positive response which is never applied. The man who longs to see his dead sweetheart, but without belief in any such possibility, is perpetually rehearsing loving greetings which he never assigns a place in his plan. There is no occasion in his life when he enacts these greetings, nor any occasion which he is prepared to meet thus and thus alone. His hopeless longing is an unscheduled response, one that has no place in the programme which he is carrying out. Or consider the opposite case of dread, the positive expectation of that which is contrary to one's will. This means that one's fixed response to a certain situation, such as the loss of money, is one of grief. To dread the loss of

money means, then, that this grief is set for the reading of the financial news in the evening paper. It is *now* set, and in some degree partially excited, which distinguishes this situation from the situation that might be described in the words "I expect to be sorry tomorrow." In the latter case one is now implicitly enacting not the sorrow but that response with which I am accustomed to deal with my sorrow, which may be to read a detective story.

There is another and quite different case to which the term dread is sometimes applied, the case, namely in which one acts to avert a future contingency. Here the belief and the interest are likely to be congruent, though there is commonly much vacillation. The familiar and apparently simple case of the burnt child's dread of fire is in reality one of the most paradoxical of all forms of behavior. We may suppose inherited connections between the visual fire-stimulus and the withdrawing movement. Suppose the child to have completed the two reflexes in the usual order: (1*a*) seeing fire, (1*b*) touching fire; (2*a*) feeling pain; (2*b*) withdrawing hand. Then in future when the visual fire-stimulus occurs it should by habit and association arouse the reaching response with the withdrawal response held in readiness and awaiting its turn. As a matter of fact, however, the child *avoids* the fire, that is, the reaching response is somehow anticipated and annulled by the withdrawing response. According to the classic explanation (the so-called "Meynert scheme") this is due to the arousal by association of the *idea* of the fire, which releases the withdrawal response in time to forestall the reaching response.[11] A simpler explanation would now be offered in terms of the "conditional reflex." This visual stimulus of the bright flame, being present *with* the pain-stimulus, becomes itself a sufficient stimulus for a retraction-movement which was originally coupled only with the pain-stimulus.[12]

[11] *Cf.* James: *Principles of Psychology*, Vol. I, pp. 24–27. For a criticism of this explanation, *cf.* E. H. Holt: *The Freudian Wish*, pp. 69–74. Holt explains the behavior by supposing that since the pain-stimulus is an intense form of the heat-stimulus, the former or the stimulus to retraction is present in some degree whenever the child is in proximity to the flame. Learning to avoid fire consists then in increasing the openness of the motor path until the retraction response is more easily aroused, or aroused at just that point in the increase of the warmth-pain stimulus that is compatible with safety and interferes to a minimum extent with the organism's other dealings with the object. This explanation is in the judgment of the present writer too limited in application to get to the root of the matter. It would not, for example, explain why a cut child dreads knives or learns not to "monkey with a buzz-saw."

[12] For a summary of "conditioned motor reflexes," *cf.* J. B. Watson: "The Place of the Conditioned Reflex in Psychology," *Psychol. Rev.*, 1916, Vol. XXIII, pp. 94–97; *Psychology from the Standpoint of a Behaviorist*, 1919, p.

In any case the important thing to note is that the visual stimulus itself causes retraction, so that the occurrence of the pain-stimulus is not necessary. The visual stimulus is not so reacted to as to introduce the pain-stimulus as a condition of the retraction response. The organism has not the purpose of removing pain, but (in so far as there is anything tentative in the behavior) of withdrawal from the visible fire.[13]

There are several ways in which the factor of purpose might enter into such a situation. The pain-reflex itself may assume a tentative or conative form. The first retraction may fail to remove the pain stimulus, and in this case the checked response will lead to auxiliary random efforts—to the casting about for means. Stimuli promising relief will be reacted to because the anticipatory act which they arouse agrees with the impeded relieving response.

A different situation arises when the pain-stimulus itself is prospective instead of present. There is undoubtedly such a thing as acting from dread of pain, or for the purpose of avoiding pain. Avoidance or prevention is "better than cure"; it is not a means to cure. We *may*, to be sure, suppose a case in which remedial action itself, the relief from distress, the actual efforts or performances incidental to the removal of a stimulus, should tend to expression in the absence of the stimulus; and in which, therefore, opportunities promising to provide the stimulus should be seized as providing an outlet for the tendency. Something of this sort seems to be the case with the longing which retired soldiers have for the revival of those very enemies, fears and privations which when they were present those same soldiers did all in their power to remove. Or there may be a virtuosity in remedial action which moves a man to look for trouble in order to give himself the satisfaction of overcoming it. But if there is such a thing as has been described, at any rate it is not what we mean by *dread* or *prevention*. In this latter case the remedial action, that is the pain together with the characteristic removal-reactions to which it gives rise, is just what is avoided. We must in this case suppose either one or both of two things: either a specific countertendency, a strong internal resistance to the play of

212. It must be admitted that since the actual mechanisms by which the conditioned reflex is created are obscure it is impossible to deny that the associative centers are called into play in some such manner as is schematically indicated in the "Meynert scheme."

[13] In order to understand the rapidity with which the conditioned reflex is established in such cases it is probably necessary to suppose that pain-reflexes have a peculiar power to persist and to spread. *Cf.* Sherrington's statements in Shäfer's *Physiology*, 1900, Vol. II, p. 974, quoted by C. T. Herrick, in *Introduction to Neurology*, 1918, p. 287.

this system of response; or a positive tendency on the part of some other and antagonistic system. In the first case, whatever response is prospectively congruent with this system will meet with the same resistance. One's purpose will be wholly negative in the sense that it rejects but does not select. If one crosses the street to avoid meeting a person one dislikes, the dread of meeting the person has acted only as a deterrent. It has forced the positive purpose of getting to my destination to abandon the straighter and shorter course. It is for this reason that action motivated by fear is lacking in constructive quality and is attended by unhappiness. Fear inhibits but does not inspire. Action governed by fear is a succession of rejections, of acts considered only to be abhorred—a series of mistakes and false starts. In the second case, on the other hand, a positive prospect, the antithesis to that which I dread, acts as a determining tendency, as when I take medicine not to avoid a headache but to feel well and to do those things which in the absence of headache I can do. In both of these cases interest and belief run together. The negative avoidance of damnation is accompanied by a correlative series of inhibited beliefs in damnation; the positive interest in salvation is accompanied by positive belief and takes the form of hope instead of dread.

There is one further case of the conversity of interest and belief, which occurs at the moment when the belief is tested. One may then be "pleasantly surprised," or have one's "worst fears realized." To be pleasantly surprised, as, for example, at one's election to office, means that the response prepared for the occasion of the reception of the news was one of regret, of fortitude, of redirection of activity to other objects. In spite of efforts in the past to obtain election one had subsequently arranged to deal with failure rather than with success. But although the news of one's election finds one unready, the response which one awkwardly and tardily brings into play is a positive and joyful response. The news is grateful to the ear and releases constructive activities subordinate to the political purpose which is now renewed. When one's worst fears are realized, on the other hand, one has prepared for the worst and finds that preparation suitable to the event. One's belief is verified, though one's desire is thwarted.

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RECONSTRUCTION IN MENTAL TESTS

AS to the existence of a goodly amount of unrest among psychologists and others who are working in the field of mental tests, there can be no doubt. Such unrest is natural following a period of inflation and is probably symptomatic of a tendency to get back to normal. It is, however, of some importance to watch carefully the process of readjustment, if for no other reason than that it may be as painless as possible and productive of no new disorders that are preventable.

A recent article,[1] clearly a product of this unrest, seems to be traveling over dangerous ground toward a goal that is hardly wholesome. The opinions expressed in this article are shared by enough others in and adjacent to the test field so that its contents assume an importance quite out of proportion to that of the article itself.

The general point of view which is taken is that mental testing is not a descriptive but a technical science; and that since statistical methods as applied to tests have been largely methods borrowed from the descriptive sciences, the time has come to look for fundamental revision in statistical approach. It is urged that every method must aim at the most direct and empirical solution of a problem—"no hypotheses, as thoroughly empirical treatment as may be!" Among other specific assertions, we find, "The actual distribution of various traits is a matter of academic interest only." It is contended that such a revision of methods, aimed at the practical solution of a practical problem, is necessary for the clarification of the total situation.

Before attempting to discuss the truly vital issues involved in such a point of view as this, it may be well to clear the ground by discussing briefly the concrete illustrations in the article. There it is pointed out that a school superintendent who is called upon to make a decision as to the use of this, that, or any test scale, and who is given data as to the "reliability" and "validity" of these scales and information as to the statistical procedure in the construction of this scale, will not be given the information that is really essential to him in making his decision. Of course, this is obvious. But, surprisingly, the conclusion drawn is that the study of the "reliability" and "validity" is futile, and that the construction of test series or tests that approximate as nearly as can be determined true scales of measurement is irrelevant to the prob-

[1] Pressey, S. L., "Suggestions," *etc. Psychological Review*, Vol. 27, No. 6, page 466.

lem. The criticism more properly belongs to the advisor of the school superintendent who suggested these three criteria by which to judge the practical usefulness of his tests. The criticism of statistical methods on such grounds is much like finding fault with the esthesiometric index because it had been suggested as a measure of pitch discrimination; or of current quotations in sterling because of their unsuitability as an indication of the selling price of wheat.

It is worthy of note, however, that a writer in this field believes that a school superintendent might be thus badly advised. If I may be permitted to depart for a moment from the main discussion of this paper, the fact that such bad advice is obtainable is a matter which psychologists can not much longer ignore. It is the result, to my mind, of two conditions. First, inadequate instruction in statistical technique for students who are doing their work in mental tests; and second, careless editing of psychological and educational periodicals. Not only are most instructional forces rather poorly equipped from the standpoint of personnel to give adequate statistical instruction, but the libraries in many cases lack periodicals and books which are essential for the student in mental tests. To be concrete, Biometrika should be available for first hand reference by such students, and certainly by research workers.

The journals are perhaps even more responsible for the existence of questionable statistical opinion. Articles are all too common which, from a statistical point of view, are distinctly amateurish. One quotation will be illustrative. In a recent article, the subjects which were examined are being described: "In the first group, there were seven boys and eight girls. Of these, two were colored. In the second group, termed the abnormal group, there were nineteen cases. The chronological age ranged from eight to fifteen years. There were ten boys and nine girls. Of these, seven were colored children. *Thus, the two groups represented a chance selection.*" Fifteen pages of the journal were consumed in reporting an experiment based upon such a selection of subjects.

Another type of article which frequently appears is that which naïvely expounds as new some bit of statistical technique which has long had a recognized and reputable standing—without ever a word as to its origin. Thus, we have an article telling how to weight the individual parts of a test to gain maximum diagnostic value without any mention of Spearman's work which was published in 1913. We find articles telling us how to compute a correlation without the use of products, without any acknowledgment of Pearson's or Harris's notes appearing prior to 1910. The de-

mand is made for methods which will show how unmistakably a test scale will set off the lower 15 per cent. or so in scholastic ability; but this technique was devised by Pearson in 1909, its applicability to experimental psychology pointed out by Brown in 1911, and its suitability as an index of diagnostic value in the test field has been discussed by later writers. The authors of such papers themselves can not be held blameless; but after all it is the journals that have brought into existence the reputations to which school superintendents turn for advice.

To come back to the main subject under discussion, the desire for revising our statistical technique to conform with mental testing, not as a descriptive, but as a technical science. There are at least three good reasons why such an attitude is to be deplored.

In the first place, it leads to the giving of approval to various statistical tricks which may be expedient enough in producing results in an immediate and practical situation, but which are actually bad method. Pressey comments on the statistical methods used in the development of the army trade tests as being a step in the right direction. As a matter of fact, from the point of view of method, the trade tests' statistical technique is open to serious criticism. To be sure, the present writer must be held responsible for the statistical method which was used in constructing trade tests, but he would justify it on the ground of practical necessity. Mere practical usefulness does not appear to justify a place for this particular technique as reputable scientific method.

In an earlier article in this JOURNAL,[2] it has been pointed out that the barrenness of the field of mental tests is due in some measure to the extent to which statistical technique has been permitted to compromise the results of test measurement as descriptive material. Certainly, the creation of a "statistics" which shall be simply a tool in the solution of a technical and practical problem can not do other than accomplish further distortion of the actual phenomena which are observed. Pressey would even use his statistics to produce bi-modal distributions in the observed phenomena, because, as he says, "A normal distribution of scores is not to be desired if the greatest efficiency is sought."

To sum up my first point, it seems to me that if we make practical efficiency the criterion by which our statistical technique is judged, we shall encourage the use of innumerable methods, faulty or merely expedient, which have, to be sure, served a purpose, but which can not be genuinely productive in a scientific sense.

[2] Vol. XVII, No. 3, p. 57.

In the second place, this point of view results naturally in a tendency to ignore the necessity for analysis, and for the isolation of variable factors in connection with a test problem. Concretely, this tendency finds its expression in the existence of "omnibus" tests and the like, methods which are concerned only with the total score and the immediate desire for successful prediction.

These first two points of criticism gain added significance as they are related to a third point, namely, that this point of view, with its approval of the expeditious, with its tendency to warp the observed data in its statistical machinery, with its inclination to discard as irrelevant and academic the laborious isolation of factors and definition of concepts, must invariably limit the possibility of significant contributions to psychological science from the mental test field.

The future of mental tests, even as applied science, hinges on the capacity of the field to produce contributions that will give us more light on the general problems of mental adjustment. These contributions will be in the highest sense of theoretical importance. It is therefore, to my mind, unfortunate that there should exist a point of view towards statistical technique that will ultimately bring about increased scientific sterility of the field.

The best progress in the field of mental tests can not come from considering that "mental testing is not a descriptive but a technical science." Like any other science, the science of mental tests must be descriptive before it can be technical. We are urged to accept as a slogan: "No hypotheses, as thoroughly empirical treatment as may be!" It would be more suitable to urge at the present time, "No problem that does not test out an hypothesis."

Psychologists could well afford to ignore for the time being the practical applications of mental tests. These have been adequately established and there will always be technicians to work out refinements in practical procedure. The need to-day is for a clarification of the concepts and hypotheses underlying the mental test field; which may in time lead to the development of a theory of measurement that will be consistent with our best knowledge of mental life.

That there must be a change in mental test theory and mental tests methods is evident. The restlessness of workers in the field is certain indication that some change will come. But the change must be an orderly one, based upon scientific principles and upon the most exact methods that exist or that can be devised. "Direct action" can not hold more promise in the realm of science than

in the field of politics; and the most productive reconstruction in mental test theory and technique will be one that serves to increase the contribution of mental testing as descriptive science.

BEARDSLEY RUML.

CARNEGIE CORPORATION.

THE TWENTY-NINTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

THE twenty-ninth annual meeting of the American Psychological Association marked an unusually well attended and enthusiastic gathering of American students of psychology. Because of Chicago's central location and probably because of the fact that the American Association for the Advancement of Science also held its meetings in the midwest metropolis, the registration was unusually large. It would be rash of course to attempt to correlate the fresh and eager attitude of the assembled psychologists with the proverbial breezes of Chicago, but possibly there is some relation between the enthusiasm of the psychologists and the remedial effects of time upon the disturbances of war. The calmer consideration of problems scientific and pedagogic gave ample evidence of our passing on from war time events. In truth, the various discussions during the several sessions manifested stronger currents of psychological and general scientific interest than has been the case in the past few years.

The session began on Tuesday morning, December 28, and ended the Thursday morning following. At times the meetings were rather bewildering, since, because of the combined sessions with sections I and sometimes Q of the American Association for the Advancement of Science, there were as many as four sessions at once. All meetings were held in the Law Building of the University of Chicago. The meetings were announced in the programmes as follows: Tuesday morning a session for general psychology; in the afternoon of the same day two sessions were scheduled, one in experimental psychology paralleling a joint session with sections I and Q of the American Association for the Advancement of Science. Wednesday afternoon there was scheduled the address of Dr. Yerkes, the retiring vice-president of section I which was articulated with the psychological association meeting, advertised as a session for social psychology. Dr. Yerkes spoke on "The Relation of Psychology to Medicine," and advocated the establishment of psychology in the medical curriculum as a basic medical science.

On Wednesday afternoon the session on social psychology was followed by the business meeting. In the evening the annual dinner

of the American Psychological Association was held in Ida Noyes Hall, the palatial women's social center building of the University of Chicago. Following the dinner, the president of the Association, Dr. Shepherd Ivory Franz, delivered an excellent and important address, and later in the evening a period was devoted to some reminiscences of Wilhelm Wundt by Messrs. Cattell, Judd, Scott, and Pintner.

The programme for Thursday morning announced a session for comparative psychology. In addition to these meetings announced on the American Psychological Association programme, the American Association for the Advancement of Science programme scheduled a joint meeting of section I (psychology) and Q (education) for Tuesday morning, and a "Symposium on Problems of Psychology" for Thursday morning. The psychologists who spoke at the symposium and the titles of their papers were: Dr. Cattell, "Practical Psychology;" President Scott, "Psychology in Industry;" Professor Judd, "Problems in Psychology;" Dr. Yerkes, "Problems of Psychology."

In commenting upon the meetings it is necessary to distinguish between the session headings of the programme and the contents of the papers read. For the two things, namely, the session headings and the contents of the papers did not always agree. Thus, the session announced for comparative psychology listed only two papers out of a total of six, which were reports on animals. Also, out of seven papers listed under industrial psychology and tests, only two were industrial in nature. To these two papers may be added a third on advertising media, strangely appearing under the heading of comparative psychology; these three constituted the total number of papers on industrial and commercial topics. As no doubt there is a definite correlation between the interests and occupations of American psychologists and the content of the papers read at this meeting, it is interesting to enumerate the papers by topic. Tests and guidance, 22 papers, plus 7 in the psychological section of the American Association for the Advancement of Science; experimental psychology, 19 papers, 8 of which were on learning, 2 on auditory phenomena, 5 on oculo-visual investigations and the rest on scattered topics; general psychology, 7 papers; social psychology 3; and animal psychology 2.

Judging from the association meeting, it is not incorect to say that American psychologists are for the most part tremendously interested in tests of various sorts, although there are numerous indications of serious dissatisfaction with much of the testing work done. How decided is the disapproval of much of the test work could be gathered from the remarks of Professor Judd, who spoke in the

symposium of the American Association for the Advancement of Science. Professor Judd uttered a definite warning against much of the work in tests, making many references to dangers to psychology as a science. The theme of his remarks was that the persons working with tests are not careful enough to provide themselves with a basic scientific psychological foundation. Unfortunately Professor Judd made no specific recommendation as to what constitutes a scientific foundation for testing work, although as a general proposition his argument seemed decisive and compelling. The general impression gathered from his remarks seemed to be that he was referring to a more intense acquaintance with the principles and technique of experimental psychology, presumably in the sense of physiological psychology. This opinion was expressed by the speaker in the form of an assertion to the effect that mental testers were not giving enough attention to fundamental explanatory phases of psychological science. Now although there are few who would disagree with Professor Judd, as to his general thesis, the psychologists interested in mental test work can not apparently find any very close relation between the introspective experimental psychology and their particular problems.

Psychologists in general will be more than pleased, however, to observe the growing sentiment against the unchecked development of uncritical testing work. Dr. Yerkes, who spoke after Professor Judd in the symposium, was very positive in his remarks concerning the necessity for a critical analysis of mental testing. Dr. Yerkes referred to the fact that many testers were not psychologists at all, and were unfamiliar with psychological principles. As evidence of the precariousness of much of the testing work we might refer to the uncritical handling of such concepts as *superior* and *inferior* as applied to races in the papers on the comparative testing of American white and other subjects. The unscientific and probably unknown bias concerning the superiority of one's own race is responsible for much innocent shifting of emphasis, even when the reporter's own data are unequivocal in discouraging conclusions of superiority and inferiority.

Professor Dodge, who opened the discussion, following the presentation of the symposium papers, apparently stimulated by a suggestion made at another session that we substitute more Freudian material for the physiological psychology, made an impassioned plea for the conservation of the old materials in psychology. He connected this point with the problem of mental testing, in that he considered physiological psychology as a valuable basis for any application of psychology. From a strictly scientific standpoint the

problem of providing a critical explanatory basis for mental testing and other psychological application is not that of abiding by any tradition, as Professor Dodge's plea may be superficially interpreted, but rather to base psychological applications upon verified psychological hypotheses. And so it is significant to observe that the active work in experimental psychology is either concerned with problems in learning or with more definite determinations of the physical stimulating conditions of the person performing visual or auditory actions.

After listening to the various discussions at the several sessions, one is severely impressed by the fact that comparatively few psychologists are interested in bringing to the surface the theoretical implications of their experimental work. Thus, for example, it was possible for Professor Dodge to make his plea for traditional psychology, although his own experimental work has been for the most part directed toward achieving a better control over stimulating conditions to the end that an exact correlation could be made between such stimulating conditions and the reactions to them, and thus is not traditional at all. In other words, Professor Dodge does not take account of the differences between his own objective methods and the less valuable introspectionism. To the writer it seems that much of the difference between those who knowingly adhere to an objectivistic position and most of those who work in sensory experimental psychology, is that the former do, while the latter do not make plain to themselves that they are not working merely with manifestations, but that their data *are* the correlations which they record between definite forms of reactions and specific stimuli. This fact comes out clearly when we observe that, although the experimentalist does actually correlate stimuli and responses, he thinks of himself as studying something with which he is not dealing directly, namely, consciousness, and so the learning as well as the sensory experimentalists still believe that they must offer neural explanations for their results, although it seems a far cry from the actual learning act to a hypothetical synaptic connection. It is but natural, of course, that the nervous system should serve as the tangible parallel and representative of the invisible and elusive consciousness.

That psychologists in general are not very much interested in so-called theoretical problems is attested by the fact that little discussion followed the reading of the papers in the general session. The practical temper of the psychologists present was markedly evident since it required the stimulus of Dr. Cattell's remarks on the statistical treatment of data, in his paper on the "Validity of Votes," to invoke any comment at all. This paper, and Professor Thurston's paper on "What Should Be Taught in the Introductory Course," were the

only ones of the general session that aroused the assembled psychologists to expressive activity. Thurston's suggestion to introduce the beginning student to some phases of human behavior aside from the simple sensory reactions met with quite violent opposition. The comment following Thurston's paper indicated quite clearly that whatever may be the condition in our institutions concerning the gathering and testing of scientific information the work of teaching will be very well guarded indeed. Are American psychologists not interested in the development of new attitudes and in the interpretative correlation of psychological facts gathered in the various domains of observation? Possibly our teachers are merely depending upon the workers in other countries to initiate problems and to develop fundamental principles, for it is impossible to believe otherwise than that without such work there would soon be nothing to teach so far as principles are concerned. And who would be so imprudent as to deny the connection between psychological principles and the techniques which make it possible for us to have facts at all?

And yet the lack of interest in theoretical problems does not signify any profound absorption in experimental psychology. This fact appeared evident from the scanty attendance upon the experimental sessions, whereas the test meetings were crowded. The vogue of the metal-instrument psychology is not flourishing and for the reason that it is generally appreciated that even with metal instruments we are unable to obtain data concerning processes which are invisible and intangible. That is to say, psychologists are apparently unwilling to go on with the old problems set by the epistemological physiologists of a half century ago. But on the other hand, the lull in the development of experimental psychology is due no doubt to the failure to appreciate generally that the newer problems involving responses to stimuli can be advantageously cultivated with the aid of laboratory equipment. For by means of instruments we can certainly refine our observations of stimulating conditions and the reactions of the person. For the advantage of psychology we must note that complaint can only be made against the old parallelistic experimentation, and in truth there is to-day far too little work done in the psychological laboratory, although obviously human psychology must be largely a field science.

An irony of science it is, as we have formerly implied, that the very psychologists who hint at the lack of interest in experimental psychology, by referring to the lure of the spectacular and the attractiveness of popular applause, such as are met with by the tester and those who in general apply psychology, are themselves responsible for the backwardness of fundamental psychological experimentation. As we have indicated above, the failure to appreciate overtly

that experimental psychology is now working in the service of objective science and not still searching for a soul, and the neglect to proclaim this fact, is to the largest extent responsible for the failure of development in the experimental field. Signalized in this failure by the fact that laboratory work in psychology is quite strictly confined to the physiological sort and seldom includes a problem in the so-called higher mental processes. Although there must be great temptation in popular applause, it is only fair to say that psychologists desert the laboratory problems in order to turn to testing work, mainly because test statistics appear definite and certain. Unfortunately, however, the statistical statements are sometimes mistaken for actual psychological phenomena.

Of especial interest is it to note that the prevailing tone among psychologists who do discuss principles is objective, and the prevailing tendency is to couch discussion in psychobiological terms. Although there was visible in several papers an attempt to set one's own introspectionism against the behaviorism of others and *vice versa*, still there was so much reinterpretation of fact and principle as to allow more than a casual coming together upon the same ground. Especially noticeable was this tendency in a paper by Professor E. G. Boring on the "Common-Sense of the Stimulus-Error." This writer, while aligning himself with the introspectionists, made such an analysis of the facts of the cutaneous limen of duality, as to make his position practically indistinguishable from that of another writer, calling himself an objectivist. Professor Boring's analysis was directed against both the extreme introspectionists and the extreme behaviorists, in that it brought to light both the stimulus and the various stages in the response-situation. When such an analysis is made the difference in positions vanishes completely, although the names of the factors in a stimulus-response situation vary at the hands of the different writers. Here lies the value of actively attending to one's psychological principles, for not only does such an inventory lead to an understanding between different workers, but it also paves the way for much needed co-operation.

The keynote to current psychological thought and observation, as it appears to the writer, was sounded in Dr. Franz's presidential address. Dr. Franz entitled his paper "Cerebral-Mental Parallelism," and planned in it to indicate the uselessness of such a problem in psychological work. Dr. Franz's paper was the report of some cases which appeared rather definitely to indicate the lack of dependence of mental activity upon specific cortical centers, although of course there is a general correlation of such activity with the

cortex. The substance of the address may be summed up in two propositions. (1) The destruction of cortical tissue need not necessarily result in a destruction of mental activity. Individuals whose defective brains were studied after death had previously reported no loss of imagery. And (2) even when cortical lesions are correlated with mental disturbances the subject may experience a restoration of mental function without improvement of the cortical tissue. Dr. Franz believes that mental activity consists of a series of habits which the individual acquires. Now when some of these habits are destroyed or impaired they may be reconstituted, although the cortical lesions may remain. It is not difficult to see how this can occur, if we remember that an act can be constituted of various kinds of individual sensori-motor coordinations. In one case quoted by the speaker a surprising amount of cortical tissue was deteriorated in a person who for several years before death had enjoyed a renewal of much of his mental and intellectual activities. In the conclusion of his address Dr. Franz strongly urged the psychologists present to look favorably upon researches in physiological and neural psychology, since after all the facts seemed to indicate that psychological reactions involved the total operation of the person. To the writer it seemed that no psychologist who heard the address could fail to be impressed with the importance of its contents for the psychophysiological problem. With the study and description of more cases such as were presented in Dr. Franz's paper and with the substantiation of Dr. Franz's results there must come a general acceptance of the unitary character of the psychological reaction, and a greater appreciation of the place of the nervous centers as coordinate members among a larger series of factors constituting a reaction. Dr. Franz's paper shows the way to a confirmation of the objective viewpoint in psychology and a revival of investigation in the fundamental principles of psychology. It certainly indicates the way to a study of the reflex and general sensorial processes from another standpoint than that of the old introspective psychology.

As a most fitting memorial to the late master-spirit of psychology, Wilhelm Wundt, the president of the association, called upon several members who had had contact with Professor Wundt, to speak of his life and work. Appropriately enough President Franz first called upon Dr. Cattell, who was an assistant in Wundt's laboratory and who is himself a brilliant figure in the annals of American psychology. Dr. Cattell spoke briefly of Wundt's personal life and then gave a very impressive picture of the profound and far reaching scholar. Professors Judd and Pintner spoke mainly of the man and teacher, emphasizing the humbleness and kindness of one of the world's foremost scientists when dealing with his students. Presi-

dent Scott of Northwestern spoke impressively of the prolonged interests which Wundt maintained in any subject in which he was interested at all. Thus he pointed out how Wundt continued to revise his early works until they reached as many as six editions. It was this prolonged and vital interest in whatever he undertook that Dr. Scott suggested as the factor which above all made for the greatness of the man. A number of the reminiscences clearly indicated the long contact which Professor Wundt maintained with the development of psychology as a science. Especially was this fact brought out by Dr. Cattell's statement that in his early contact with Wundt the latter was of the opinion that psychological experiments could only be performed with trained psychologists. Genuinely instructive was the reference made by one of the speakers to the wide range of Wundt's interests. Mention was made of his ponderous works in logic and ethics, which indeed emphasizes the contrast in scope of interest among scholars which academic custom dictates in our country and in Europe.

At the business meeting held on Wednesday afternoon the election was announced of Professor Margaret Floy Washburn, of Vassar College, as president of the American Psychological Association for the year 1921. Professor W. S. Hunter, of the University of Kansas, and Professor G. F. Arps, of Ohio State University, were elected to the council of the association. The association nominated Professor W. B. Pillsbury, of the University of Michigan, and Professor G. M. Stratton, of the University of California, to represent the Association in the National Research Council, while Professor E. K. Strong, Jr., of the Carnegie Institute of Technology, is to represent the American Psychological Association in the council of the American Association for the Advancement of Science. As officers for section I of the American Association for the Advancement of Science, the election was announced of Professor E. A. Bott, of Toronto University, as Vice-president; Professor F. N. Freeman, of the University of Chicago, as Secretary, and Professor L. W. Cole, of the University of Colorado, as chairman of the section committee. The Association elected 35 new members, increasing the membership to 428.

Incomplete is a record of the twenty-ninth meeting of the American Psychological Association without recording the names of two distinguished visitors at the sessions, one, the newly inducted successor of James and Münsterberg in Harvard's department of psychology, Professor William McDougall, who was welcomed to membership in the association, and the other, Professor G. A. Jaederholm, of Sweden.

J. R. KANTOR.

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REVIEWS AND ABSTRACTS OF LITERATURE

Les Classiques de la Philosophie; publiés sous la direction de MM. VICTOR DELBOS, ANDRÉ LALANDE, XAVIER LÉON. Paris: Librairie Armand Colin.

VIII. BERKELEY: *Les Principes de la Connaissance Humaine,* traduction de Charles Renouvier. 1920. Pp. xii + 107.

IX. BERKELEY: *La Siris,* traduction de Georges Beaulavon et Dominique Parodi. 1920. Pp. viii + 159.

XII. MAINE DE BIRAN: *Mémoire sur les Perceptions Obscures, suivi de la discussion avec Royer-Collard et de trois notes inédites.* 1920. Pp. xii + 66.

Les Classiques de la Philosophie, of which three volumes have appeared, is a new series designed to put into the hands of French readers some of the philosophical classics which at present are unavailable to them. Descartes, Malebranche, Condillac, and Maine de Biran are the French authors represented in the announced list, and Hobbes, Berkeley, and Kant are those of whom translations are to appear. Since the series is intended merely to fill in gaps in the literature formerly accessible, some of the more important of the writings of those authors are not included. The price per volume is moderate, varying from three to five francs. The texts are edited critically, with carefully prepared footnotes on the various readings of the successive revisions of the works. Each volume is accompanied by a brief biography of the philosopher and an excellent bibliographical notice.

The volume of extracts from the still largely unpublished works of Maine de Biran gives us one of his main essays in psychology, and shows both his dependence upon Condillac and his departure from Condillac in the direction of mysticism. This essay is followed by four brief extracts from the manuscripts in the possession of the Institute of France, three never before published, in which extracts the attitude of Maine de Biran is shown towards four other psychological authors of his day, Royer-Collard, Bonstetten, Reid, and Dugald Stewart.

The two volumes of translations of Berkeley are interesting as evidence of the place held by Berkeley in French thought. It is startling to learn that, whereas *Siris* appeared in an earlier French translation as long ago as 1745, that is, but one year after its first publication in English, the *Principles* never was put into French until 1889. The 1889 translation is indeed the only translation ever made into French. It was made by Renouvier, and originally appeared in sections in five successive issues of *La Critique Philosophique,* of which Renouvier was at that time editor. This present

reprint of Renouvier's translation, into which only minor corrections have been introduced, is consequently the first time that that important and basic document in modern idealism has been available readily and in book form to those who read only French. It is perhaps hazardous to rest any conclusion upon these dates. But one may well wonder whether the order of translation was at all due to the congruity of the contents of the two works to the current tradition in French philosophy. Malebranche repudiated the doctrine of the *Principles* as foreign to his own thought; but he might not have objected so strongly to the *Siris* which appeared after his death and contained a more Platonic type of idealism. At least, whatever the explanation may be, the amazing facts are that, while the *Siris* was almost immediately translated, the *Principles* had to wait nearly two centuries for its first French translation and more than two centuries for the publication of that translation in book form. Considering the much greater importance of the *Principles*, one can not but wonder at these dates.

Students of Berkeley may well spend a few hours in reading Renouvier's French translation of the *Principles*. The effect produced is somewhat different from that obtained by going over the English original. This difference is due primarily to the use of *esprit* as the equivalent for *mind*. Descartes, with whom the modern psychological approach to metaphysics may be viewed as beginning, used *âme* about as frequently as *esprit*, both of which words are usually translated as *mind* by Haldane and Ross. Locke, even though following Descartes in his proof of the existence of the self, avoided the use of the word *soul*, probably because *soul* had retained a theological connotation lacking to the French *âme*. Yet *mind* had for Locke the same substantial meaning as *âme* for Descartes: It was a realm of being, a receptacle in which ideas are deposited, an order of existence ontologically different from that of external objects. And Berkeley, even while denying the existence of Locke's external objects as meaningless, retained his supposition of the substantial self, which he more often called *mind* than *soul*. Hence when the French translation of Berkeley's *Principles* employs *esprit* as the equivalent for *mind*, Berkeley's metaphysics loses much of the sense of substantial and solid being, and becomes more delicate, more airy, more *spirituel*. That is, one passes from the French *âme* to the English *mind*, and then back to the French *esprit*, with the result that idealism is not so much insistence upon a kind of fundamental substance as revelation of the importance of meaning, of implication, of logical connections. In the French translation of *Siris*, the word *mind* is consistently translated as *intelligence*, which further adds to the changed emphasis in Berkeley; yet this work is

already in English somewhat Platonic, and makes such a rendering quite justifiable. The reader of these French translations of English classics can not but be impressed with the subtility of language in the proper rendering of ideas; and he may wonder how often in the history of philosophy certain views have been adopted because of the words available for expression.

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JOURNALS AND NEW BOOKS

REVUE PHILOSOPHIQUE, July-August, 1920. *La sensibilité, l'intelligence, et la volonté dans tous les faits psychologiques* (pp. 1–57): FR. PAULHAN. – "Sensibility, intelligence, and will are not . . . groups of facts, well delimited and separated from one another, any more than they are products of three distinct metaphysical faculties." They are to be found everywhere in the life of the spirit. *L'imagination pure et la vie esthétique* (pp. 58–87): J. SEGOND. – "Esthetic life represents in its own fashion and implies in its work a kind of mathematics of quality . . . a kind of physics of quality . . . a kind of history of pure quality." *Matière et société* (pp. 88–122): M. HALBWACHS. – An analysis in defense and clarification of the following definition of the working class: "the group of men who, in order to acquit themselves of their task, must turn themselves towards matter and pass out of society." *La Scolastique* (pp. 123–141): P. MASSON-OURSEL. – Through comparative study of occidental and oriental scholasticism seeks to show that scholasticism is not merely an episode but a necessary phase of thought. Oriental scholasticism like the occidental form possesses three chief characters: "the exposition of thought under the form of commentary, the dialectic method, and the belief in the value of systematization, with this corollary: the position of the philosophical problem as a classification of categories." *Analyses et Comptes rendus.* Ettore Galli, *Nel regno del conoscere e del ragionare:* R. GUÉNON. Columbia University, *Studies in the History of Ideas:* P. M-O. Irving Babbitt, *Rousseau and Romanticism:* P. MASSON-OURSEL. E. Seillière, *Les origines romanesques de la morale et de la politique romantiques:* P. MASSON-OURSEL. Th. Ziehen, *Lehrbuch der Logik auf positivistischer Grundlage, mit Beruecksichtigung der Geschichte der Logik:* P. MASSON-OURSEL. Léon Daudet, *Le monde des images:* FR. PAULHAN. J. Varendonck, *La psychologie du témoignage:* P. MASSON-OURSEL. J. Varendonck, *Recherches sur les sociétés d'enfants:* E. CRAMAUSSEL. Pierre Dufrenne, *La réforme de l'école primaire:* E. CRAMAUSSEL. *Revue des périodiques.*

Durant, Drake, Arthur O. Lovejoy, James Bissett Pratt, Arthur K. Rogers, George Santayana, Roy Wood Sellars, C. A. Strong. Essays in Critical Realism: A Coöperative Study of the Problem of Knowledge. London: Macmillan & Co. 1920. Pp. vii + 244.

Foster, George Burman. Christianity in Its Modern Expression. (Edited by Douglas Clyde Macintosh.) New York: The Macmillan Co. 1921. Pp. xiii + 279. $3.75.

Hollander, Bernard. In Search of the Soul, and the Mechanism of Thought, Emotion and Conduct: A Treatise in Two Volumes containing a Brief but Comprehensive History of the Philosophical Speculations and Scientific Researches from Ancient Times to the Present Day, as well as an Original Attempt to Account for the Mind and Character of Man and Establish the Principles of a Science of Ethology. London: Kegan Paul, Trench, Trubner, & Co. New York: E. P. Dutton. No date. Pp. 516, 361. $20 per set.

NOTES AND NEWS

A MEETING of the Aristotelian Society was held on February 7, Lord Haldane, vice-president, in the chair. Professor R. F. A. Hoernlé read a paper on "A Plea for a Phenomenology of Meaning." The task of a phenomenology of meaning is to collect and examine all types of empirical situations in which signs function and meaning is present. This is the more necessary as all the higher activities and all control of social organizations depend on the use of signs. Yet current theories are fragmentary and one-sided. This is shown by an examination of the theories of F. C. S. Schiller, B. Russell, Lady Welby, C. S. Peirce, G. F. Stout, A. Meinong, and E. Husserl. A clue to a completer theory may perhaps be found in the distinction between the *indicative* and the *expressive* function of signs. We have the pure indicative function when the existence of *A* enables us to infer the existence (or non-existence) of *B*. We have the pure expressive function when an agent makes, or utters, signs. The two functions are curiously interlaced in intersubjective intercourse. The distinction, however, requires to be further tested by application to various kinds of non-verbal signs, to symbolic actions, and especially to the functions of sounds in music.

THE Department of Psychology of the University of Oregon announces that it has met all requirements established by the Faculty of the University, concerning library facilities, laboratory equipment, courses and scholarship, and is now authorized to offer work for the degree of doctor of philosophy.